

results from this is not quite apparent to an outsider: it is even probable that there is none, unless the unintended reflex benefit, in the form of experience in book-making, which the authors thereby obtain.

Here we have two elementary booklets, one of 40 pp., the other of 28 pp.; and a very short examination of them suffices to show that the writers could have spent their time and energy to much better purpose, if it was the public that they intended to benefit. What they have written is probably not worse than what has been in use for years; but certainly it is not any better. Indeed Germany has always had more really good elementary expositions of the theory of determinants than any other country, and two or three of these have passed through several editions. Dr. Kaiser and "Professor" Bunkofer are quite capable men for the work they have undertaken: on this score little fault can be found. The latter sketches his "Notes of Lessons," as young English teachers would call them, with pedagogic ability and skill; the former is more wooden, and more unwisely ambitious, and we cannot, unsupplemented, pardon him for saying that Gauss in coining the word "determinant" thereby introduced a definite new idea into analysis, but he goes about his work in a sufficiently workmanlike manner, and is on the whole sure of the ground he treads. We only wish both authors "more power," and next time a happier selection of subject.

Prof. Mansion's "Elements" is a book of a higher type. The present edition, however, is the fourth; and therefore no detailed examination can be looked for. Suffice it to say that there is really no better introductory book published; the exposition and arrangement are admirable, and it has, what so many Continental text-books want, small collections of suitably graduated exercises for the learner. There is only one point which it seems desirable that Prof. Mansion should reconsider, viz. the nomenclature of the special forms of determinants. He employs, for example, both Sylvester's term "persymmetric" and Hankel's "orthosymmetric." Should not one of these immediately receive decent burial, and should not the latter be that one? It is not shorter, it is not more descriptive, it is not more accurate in its description than its rival, and its rival was by several years first in the field. As for "doppelt-orthosymmetrisch," its author is simply unconscionable; it is one of those words which, as Mark Twain puts it, are alphabetical processions and have a perspective: we should have been glad if Prof. Mansion had dealt more summarily with it. In another instance, that of "skew" determinants, we have confusion worse confounded. Cayley's first paper regarding them appeared in *Crelle* (1846), and was written in French, the title being "Sur quelques propriétés des déterminants gauches." The term *gauche* (Eng. *skew*, Germ. *schief*, Italian *gobbo*) was at once accepted and employed, as well it might, by all the standard writers. Of late years, however, there have been busy times with the mathematical coiners on the Continent, and in consequence we have as substitutes for "skew"—

"symmetrale,"  
"congruente" (not in Mansion),  
"pseudosymétrique."

Surely it is too tiresome and quite unnecessary to wait until by a process of artificial selection the fittest or un-

fittest of these shall survive. Prof. Mansion's "Elements" and the German translation of it have deservedly a large circulation on the Continent, and thus have much power to propagate good or evil. We would therefore earnestly ask him to consider whether it would not be better to recognise throughout his work only *one* name for each special form, and to relegate all synonyms to the index.

The last text-book on our list is Spanish. Although it is the largest (200 pp.) and most pretentious of the four, we regret that it is impossible to say a good word regarding it. The authors have most manifestly no grasp of the subject, and advance with a gay step and light heart through inaccuracy after inaccuracy. Their model unfortunately is Dostor, and equally unfortunately they are more than faithful to him. At the very outset they show their hands. The so-called "notation of Cauchy" is not Cauchy's; what is really Cauchy's is not attributed to him; and the "notation of Leibnitz" is more Cauchy's than Leibnitz's, but belongs to neither. Nor is this wild start of Book I. redeemed by a good end. On pp. 96-98 five examples of skew determinants are calculated at length with a complacent unconsciousness of the simple property which makes all the calculation unnecessary; and pp. 99-101 are taken up with the rather epoch-making definition—

$$\begin{vmatrix} a & b & c \\ a' & b' & c' \end{vmatrix} \equiv \begin{vmatrix} 1 & 1 & 1 \\ a & b & c \\ a' & b' & c' \end{vmatrix},$$

and some perfectly legitimate deductions from it. Book II. deals with the so-called applications of determinants, and closely follows Dostor. The most amusing part of it, as is the case also with Dostor, is the chapter devoted to "Applications to Trigonometry." Dostor, however, is outdone on his own ground. For example, after it has been proved that  $\cos A = (b^2 + c^2 - a^2)/2bc$ , one whole octavo page is occupied in showing, by means of determinants, that  $\sin \frac{1}{2} A = \sqrt{(s-b)(s-c)/bc}$ . This *tour de force* is like that of Hudibras, *telling the clock by algebra*; and the moral in both cases is the same.

#### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

#### Mr. Lloyd Morgan on Instinct

I HAVE read with much interest Mr. Lloyd Morgan's very able paper on "Instinct" in the current issue of *NATURE*, and I feel it is desirable, without following him over all the ground which he has traversed, briefly to consider those parts of his communication which have special reference to my own work.

The broad question with which he begins—viz.: "Is there a science of comparative psychology?"—is not a question which I feel specially called upon to answer, inasmuch as almost every one who has hitherto written upon psychology has taken it for granted that there is such a science. Nevertheless I may state the justification which I am myself prepared to give of this universal assumption.

When we say that a dog is a more intelligent animal than a sheep, we do not doubt that we are making as real a proposition as when we say that the President of the Royal Society is a more intelligent man than Dick, Tom, or Harry. Now in all cases where there is a general consensus of feeling of this kind, there is

an antecedent presumption that the common sense of which it is the expression is in the right, and that any ingeniously-constructed argument of scepticism is in the wrong. We may therefore approach Mr. Lloyd Morgan's argument with the antecedent presumption that there must be something wrong about it somewhere; and I do not think that it requires much reflection to see where the error lies.

According to the argument as stated by my critic, there is a true science of human psychology, because, although my knowledge of another human mind is no less ejective than is my knowledge of a dog's mind, yet "by means of language human beings can communicate to each other the results which each has obtained, and each human being is able to submit these results to the test of subjective verification." But how, let us ask, in its last analysis is this verification obtained? By language, no doubt; but what in its last analysis is language? As spoken by my neighbour, it is for me nothing more than my own interpretation of a meaning presented by the observable activities of an organism. Therefore, if on such a basis I am entitled to affirm that such interpretations as I make are of the nature of "subjective verifications" of conclusions drawn from the introspective observation of my own mind, why am I not entitled to a similar view when the effect of my contemplation is the mind of a dog? The dog cannot speak, but he can display other activities which, so far as they go, are quite as valid as a basis on which to construct my "subjective verification" as are the activities manifested in language. Of course language is able to convey immeasurably more information touching the ejective mind than can be conveyed by any other kind of activity; but this fact is merely due to the further fact that language is a system of activities expressly designed for this very purpose. The higher value of language in this respect is therefore nothing more than an expression of the higher development of intelligence, which enables the mind to perceive the desirability of devising a system of bodily activities expressly designed to serve as the vehicle of communication between subject and object,—as is proved by the fact that any system of bodily activities which may be agreed upon (such as gesture, lip-reading, writing, &c.) are alike available for this purpose. Language, then, of any kind is merely a conventional system of bodily activities which, because intended to convey information from mind to mind, we call signs. But now, the element of intention on the part of my neighbour is in no wise essential to my ejective interpretation of his bodily activities, or to what Mr. Morgan calls my subjective verification of them. The involuntary groan of pain, the pallor of fear, and a thousand other unintended "expressions of the emotions," as well as a thousand other unintended expressions of thought (e.g. the act of pocket-picking under the eye of an unseen detective), are, as it is proverbially said, "more eloquent than words."

I submit, therefore, that, although a dog cannot give us any large measure of ejective information intentionally, or by purposive signs (he does, however, give us some even of this), we have still abundant material furnished by his other bodily activities for constructing our ejective inferences. For example, the dog gives very much the same indications of pain under the whip that a boy gives under the cane; therefore the gamekeeper has no more doubt that he is hurting the dog than the schoolmaster has that he is hurting the boy—nor would the schoolmaster be more satisfied on this point by asking the boy whether the cane did hurt.

If I have been followed thus far, I should be inclined to go still further, and to say that in my opinion the "unpremeditated act" of natural movements (whether in men or animals) is a surer basis on which to build ejective conclusions than is the more indirect information supplied by intentional gesture or language, so far as the low or simple intelligence to which animals attain is concerned. Poets and moralists are fond of insisting upon this point as regards young children, where the level of intelligence may be even considerably above that of the most intelligent animal. The immense service of language in ejective analysis is rendered in those higher and more complex regions of intellectual life to which man alone attains. Still, I doubt not that if animals could speak, so that we could interrogate them as to their mental operations, we should obtain a great deal of supplementary information; only of course this supposition is an impossible one, seeing that, if an animal could speak, its intelligence would no longer be "animal intelligence."

On the whole, then, as concerns the question whether there is a science of comparative psychology, I should say that there certainly is such a science, in the same sense as there is a science of

human psychology. For it seems to me, in view of the above considerations, that the argument adduced by Mr. Lloyd Morgan against the former is quite as applicable against the latter. In both cases alike our ejective inferences can only be founded on the observable activities of organisms, and if it is true that of these observable activities language affords an exceptionally meaning class, it is also true that where language is absent the mental processes which stand to be ejectively analysed are of a comparatively simple nature. I therefore see no reason to recede from the position which I have taken up in the works to which Mr. Lloyd Morgan refers, where I observe with reference to the peculiar standing of psychology (both human and comparative) among the sciences in the matter which we have been considering—"although the evidence derived from ejectives is practically regarded as good in the case of mental organisations inferred to be closely analogous to our own, this evidence clearly ceases to be trustworthy in the ratio in which the analogy fails; so that when we come to the case of very low animals—where the analogy is least—we feel uncertain whether or not to ascribe to them any ejective existence" ("Mental Evolution in Animals," p. 22). And again, with reference to such objections as that of Mr. Morgan—"Scepticism of this kind is logically bound to deny evidence of mind, not only in the case of the lower animals, but also in that of the higher, and even in that of men other than the sceptic himself. . . . This is evident because, as I have already observed, the only evidence we can have of ejective mind is that which is furnished by objective activities; and, as the subjective mind can never become assimilated with the ejective, so as to learn by direct feeling the mental processes which there accompany the objective activities, it is clearly impossible to satisfy any one who chooses to doubt the validity of inference, that in any case, other than his own, mental processes ever do accompany objective activities" ("Animal Intelligence," p. 16). And, by parity of reasoning, the same argument may be used against Mr. Morgan's sceptical objection to comparative psychology as a science. In whatever measure he is on principle a sceptic touching the inferences which this science is able to draw as to the existence and nature of animal psychology, in that measure I think he ought in consistency also to be a sceptic with reference to the same points in the science of human psychology.

Coming now to Mr. Morgan's strictures on my psychological definition of instinct, I understand that they are made, not with reference to any defect in my definition as a psychological definition, but with reference to the possibility of any such definition whatever. In his view there can, from the nature of the case, be no psychological definition of instinct; there can only be a physiological definition of the cerebral processes which are concerned in actions termed instinctive. Here, then, is a broad issue, although it only constitutes a part of the still broader one which we have just been considering.

I may say first of all that, if we want a physiological definition of instinct, I do not think that the one which is furnished by Mr. Lloyd Morgan is valid. This definition is that reflex actions are due to a general type of nervous organisation, instinctive actions to a specific type, and intelligent actions to an individual nervous organisation. Now, this threefold definition presents none of that "definiteness of application" which Mr. Morgan implies, nor does it tend, as he supposes, to add any "clearness to our ideas concerning the things of which we speak." For it is open to the fatal objection of arbitrarily classifying as instinctive many actions which are now universally regarded as reflex; while, conversely, a still greater number of actions now universally regarded as instinctive would, under this definition, become classified as reflex. That is to say, there are, on the one hand, many reflex actions which we should all feel it absurd to call instinctive, and which are nevertheless manifested by only one species (in our own organisations, for example, we may mention the "patellar reflex," and the convulsions produced by tickling the soles of the feet); and, on the other hand, there is a very much greater number of instinctive actions which we should all feel it absurd to call reflex, but which are nevertheless manifested by many species of a genus, others by many genera of an order, and so on, until in such cases as those of nidification, incubation, &c., we arrive at instincts general to a whole class. The truth, therefore, is that a zoological classification, being made with reference to the whole organisations of animals, has no such special application to the refined structure of their nervous systems (which, indeed, we can only appreciate by its effects on conduct) as would be required for the groundwork of



Mr. Morgan's physiological definitions of reflex action, instinct, and intelligence. If we want such a definition it must be made independently of any zoological classification, and with exclusive reference to the point whether the adaptive action requires for its performance the operation of the higher nerve-centres—a point which can only be determined by vivisectional experiment. In other words, on the side of objective psychology the only distinction that can be drawn between a reflex and an instinctive action, is as to whether the action can be performed by the lower nerve-centres alone, or requires likewise the cooperation of the higher nerve-centres. And this is just what we should expect to find to be the case on the objective side if, as I have endeavoured to show, the one peculiarity which distinguishes actions classed as reflex from actions classed as instinctive, consists in the latter exhibiting in their performance a mental or conscious element which is not exhibited in the former.

Now, if the *raison d'être* of the term "instinct" is thus to denominate a class of adaptive actions in which there is a subjective, or rather let us say an ejective element, I cannot see that anything but confusion is to be gained by forcing this term into objective implications. Were any term needed to designate the neurosis of instinctive action, it would be far better to coin a new one than thus to abuse an old one. I am fully sensible of the difficulty which often arises in deciding whether a particular action should be assigned to the instinctive or to the reflex class; but, as I observe in "Mental Evolution in Animals," "this difficulty does not affect the validity of the classification any more, for instance, than the difficulty of deciding whether *Limulus* should be classified with the crabs or with the scorpions affects the validity of the classification which marks off the group Crustacea from the group Arachnida."

For the rest, Mr. Morgan's criticism on my psychological definition of instinct hangs entirely upon his previous criticism as to the possibility of a science of comparative psychology, and as I have already endeavoured to answer the latter, I need not go over the same ground again by answering the former. There are only two points raised by his paper to which this general answer does not apply, and with these, therefore, I shall conclude.

The first of these two points is a charge of inconsistency. My critic observes that, after having said "it is enough to point to the variable or incalculable character of mental adjustments as distinguished from the constant and foreseeable character of reflex adjustments," I go on to define instinctive actions as mental adjustments which are nevertheless of a constant and foreseeable character. Now I think, if any one will read my chapter on "The Criterion of Mind," he will see that this apparent inconsistency is not a real one. It would be a real one if the passage above quoted referred only to this and that particular action of an animal, apart from all the other actions of the same animal, which, according to my criterion of mind, are competent to inform us whether or not the animal in question is a *choosing* and *perceiving* animal. But the passage quoted refers to the whole constitution of an animal so far as we can know it by observation of activities, and therefore the question whether this or that particular activity is to be regarded as mental or non-mental (instinctive or reflex) requires to be answered by all that we learn concerning the other activities of that animal. If none of its activities are other than those of a constant and foreseeable character, we have no reason to suppose that it is a *choosing* or *perceiving* animal; but if some of its other activities are indicative of choice and perception, our knowledge of this fact must be allowed due weight in any attempt that we may make at classifying this or that particular action as reflex or instinctive. The case, in short, is just the converse of that which I thus state in the chapter referred to:—"Many adjective actions which we recognise as mental are, nevertheless, seen beforehand to be, under the given circumstances, inevitable; but analysis would show that this is only the case when we have in view agents whom we already, or from independent evidence, regard as mental."

The second point to which I have referred as the only one that now remains for me to consider, is to the effect that I have mistaken "Mr. Spencer's position with regard to the 'very subordinate importance of natural selection as an evolving source of instinct,' and with regard to the question of 'lapsed intelligence.'" Here I can afford to be brief, inasmuch as any one who cares to do so can compare my interpretation of Mr. Spencer's writings with the passages in those writings to which I refer. It seems to me perfectly clear that, although both the principles in question are alluded to by Mr. Spencer, neither of them holds the same pro-

minence in his theory of the development of instincts from reflex action as they hold in the theory of Mr. Darwin.

In conclusion, I trust Mr. Morgan may feel that, in writing this somewhat elaborate reply to his criticism, I am marking as emphatically as I can my sense of its ability. And if the general effect of this discussion is to show that the phenomena of instinct present peculiar difficulties to any attempt at a fundamental analysis, I should like no less emphatically to express my conviction that such an analysis is not to be facilitated by closing our eyes upon the entire class of phenomena to which alone the word is applicable. We may, of course, abstain from any attempt at such analysis, and devote our attention exclusively to the physical as distinguished from the mental side of the subject. Only in this case we may not speak of *instinct*.

GEORGE J. ROMANES

### "Mental Evolution in Animals"

MR. ROMANES' comment on my communication in NATURE of February 7 (p. 335) is not quite satisfactory. I do not suppose that he has any spite against my skate; but as he does not know me, and did not see the incident in the Manchester Aquarium, I think it is very possible that he may have been naturally predisposed to underrate the significance of the story. I do not admit that I can be reasonably blamed for saying that a repetition of the conditions would have been useful, if possible, while at the same time pointing out that the result would not necessarily have settled the question. Test experiments are always useful, even if they do not settle the main question. Mr. Romanes' terrier story was not necessary to make clear what he means by "accident," and there is no analogy between it and my skate story. In one case a trained, or at least tamed, dog did as he was told, and the conditions of success were prearranged; in the other, a fish spontaneously did something for his own advantage. As for the fish smelling the food, this does not harmonise with the circumstances as I described them, and had Mr. Romanes seen the incident I do not think this explanation would have occurred to him; the whole series of actions was too rapid, and had too much the appearance of co-ordination. The propulsion of the food into the ready mouth was the work of an instant. Had the mouth not been ready, as the cricketer's bat is the instant the ball leaves the bowler's hand, the morsel would have been missed. Finally, Mr. Romanes tells us ("Animal Intelligence," p. 351) that the bear observed by Mr. Hutchinson was a Polar bear. Now this species is "almost marine in its habits." It lives upon seal-flesh and also upon dead meat which it finds floating in the water. It is not infrequently cast adrift on an ice-floe or an iceberg. It is therefore not at all improbable that the method of fishing described may be an instinct developed hereditarily. The fact that two bears behaved in precisely the same manner strengthens this supposition. Mr. Darwin does not say whether the bear observed by Mr. Westropp in Vienna was a Polar bear or not, but he observes that the action in question "can hardly be attributed to instinct or inherited habit," as it would be "of little use to such an animal in a state of nature." It seems to me that such action would be very useful to Polar bears in a state of nature.

Manchester, February 11

F. J. FARADAY

### The Remarkable Sunsets

AT the present stage of the discussion upon the "green sun" and rosy sunsets it seems to me it would be well to recall attention to a few facts, for there seems to be a tendency on the part of some correspondents to allow imagination to carry them beyond the region of fact into that of fancy. First, then, I would point out that my observations show conclusively that at the time of the green sun there was an altogether abnormal amount of moisture in the upper regions of the atmosphere, while the ordinary hygrometric observations showed the air near the ground to be comparatively dry. I have studied the rain-band spectrum almost daily for the last six or seven years, and I have never before known such a long continuance of the heavy rain-band in a comparatively clear sky—a sky in which there was only a light haze. At sunset and sunrise the intensity of the bands was such as I have before seen only from an altitude of some six or seven thousand feet, and even then rarely. In this connection it may be well to point out that the spectrum as observed by Mr. Donnelly (NATURE, vol. xxix. p. 132), though, as remarked by Mr. Lockyer, resembling that observed here in